VICTIMIZATION AND THE NATIONAL CRIME SURVEY: PROBLEMS OF DESIGN AND ANALYSIS*

by

Stephen E. Fienberg**
University of Minnesota

Technical Report No. 291

June 1977

*A version of this paper was presented at the Second Survey Sampling Symposium at the University of North Carolina at Chapel Hill, April 14-17, 1977.

ABSTRACT

The National Crime Survey (NCS) was begun in 1972, with funds from the Law Enforcement Assistance Administration, to develop new information on the incidence of crime and its impact on society. The survey includes personal interviews at 6-month intervals of approximately 60,000 households, and of the residents therein, making it one of the largest and most expensive ongoing surveys in the United States today.

This paper begins with the description of a single hypothetical incident in order to clarify the differences between offense and victim rates, and to motivate the concept of a survey of victims of crime. Subsequent sections review the design, execution, and analysis of the NCS survey, and describe some models for analyses of NCS data. The paper concludes with a brief discussion of the basic design structure of large-scale social surveys.

KEY WORDS AND PHRASES: Longitudinal analysis; Panel studies; Point processes; Social surveys; Survey design; Victimization.

Author's Footnote and Acknowledgement

**Stephen E. Fienberg is Professor and Chairman, Department of Applied Statistics, School of Statistics, University of Minnesota, St. Paul, MN 55108. This article grew out of material discussed in the Workshop on Criminal Justice Statistics held in Washington, D.C., July, 1975, and sponsored by the Social Science Research Council Center for Coordination of Research on Social Indicators and the Law Enforcement Assistance Administration. The author is indebted to several of the participants of the Workshop whose ideas and suggestions inevitably have found their way into this article. In particular thanks are due to Albert D. Biderman, Kinley Larntz, Albert J. Reiss Jr., David Seidman, and Richard Sparks.

1. INTRODUCTION

Crime and its impact on society have long been the subject of public interest and social concern. While the study of crime has proved profitable to social scientists over the years, the limitations of police crime statistics (e.g. see Biderman and Reiss 1967) have always been viewed as being so great as to make it virtually impossible to measure criminality in a population. Hood and Sparks (1970) note that "Questions about criminality, like those about sexual behavior, are especially liable to distorted and untruthful answers." Thus it was with great anticipation that the social science community heralded the adoption of survey research methods to find the victims of crime, and to learn of their experiences. As a result of some small-scale attempts at victim surveys in the United States and Great Britain, and after considerable planning and preparation, the Law Enforcement Assistance Administration (LEAA) initiated a major new social statistics series based on a national victimization survey.

The primary purpose of the national victimization survey, as stated in a planning document developed by LEAA, is:

> to measure the annual change in crime incidents for a limited set of major crimes and to characterize some of the socio-economic aspects of both the reported events and their victims.
>
> (Penick and Owens 1976, p. 220)

Henceforth, we refer to this survey as the National Crime Survey (NCS), but the reader should bear in mind that the focus of the NCS is upon victims and their experiences with crime, not on crime itself.

Actually the NCS consists of four separate surveys: (1) a continuing national survey of household locations, (2) a continuing national survey of commercial establishments, (3) a separate set of single or duplicated surveys of household locations in selected cities, (4) a set of city commercial surveys to parallel (3). In this paper we restrict our attention solely to the continuing national survey of household locations.

The NCS has been designed and executed for LEAA by the U.S. Bureau of the Census and it includes personal interviews at 6-month intervals with individuals in up to 65,000 households. Given the magnitude of the NCS and the massive files of data collected since the initial field work began in mid-July of 1972, it is remarkable that the NCS has received so little attention from professional statisticians outside of the Bureau of the Census.

Central to an examination of victimization and the concepts underlying the NCS is the notion of a crime or criminal incident and how it gets recorded by various criminal justice agencies. The dictionary definition of crime offers little in the way of a starting point. For example, a recent edition of the Random House Dictionary defines crime as:

> an action or an instance of negligence that is deemed injurious to the public welfare or morals or to the interests of the state and that is legally prohibited.

To shed some light on this matter, Section 2 describes in detail a single criminal incident, and notes how it would be recorded in statistics gathered by the police and in the NCS.

Section 3 contains a brief summary of the survey and questionnaire design of the NCS, and describes some aspects of its execution. Special attention is focused on the panel structure of the survey design, with a rotation plan for households. The major shortcomings of the design are then noted. Section 4 is brief and it summarizes the published analyses from the NCS. The lack of LEAA resources devoted to the statistical analyses of NCS data was one of the principal findings of the Panel for the Evaluation of Crime Surveys appointed by the Committee on National Statistics (Penick and Owens 1976, p. 3). This report contains considerably more detailed descriptions of the NCS survey and questionnaire design than we provide here. It describes the developmental research behind the design, and it suggests areas for further investigation. The report's conclusions overlap considerably with ours regarding the need for extensive ongoing methodological research.

Any assessment of the NCS must look closely at its objectives and determine to what extent they are being met. The primary purpose

of the NCS as described above actually has several components:

1. To measure the incidence of crime.
2. To measure the changes in crime rates over time.
3. To characterize socio-economic aspects of criminal events and their victims.

Closely related to item 3 are the aims

4. To identify high-risk subgroups in the population and to estimate the rate of multiple victimization.
5. To provide a measure of victim risk.

From its inception the NCS was viewed as a multi-purpose survey that would produce not only the general-purpose victimization rates described above, but also data for policy-oriented problems, e.g.

6. To calibrate the Uniform Crime Reports data produced by the FBI.
7. To index changes in reporting behavior.
8. To measure the effectiveness of new criminal justice programs. (The city surveys were initiated for exactly this reason.)

To determine if the NCS properly fulfills aims 1 through 4, special attention needs to be focused on questions that utilize the longitudinal structure of the NCS. Section 5 outlines a number of substantive questions regarding victimization and victim-survey methodology that in principle should be answerable by analysis of NCS data. A major stumbling block to the successful completion of

these analyses is the highly complex NCS survey structure, designed to produce descriptive statistics rather than data amenable to analytical studies of interrelationships and their changes over time. Although the NCS is a rotating panel in form, the primary purposes of the panel structure are to get more stable rate comparisons from one period to the next, and to bound the time frame under consideration.

2. RECORDING CRIME

Criminal incidents are events or social encounters involving one or more offenders and one or more victims, in one or more locations for specified periods of time. The duration of a single criminal incident may be 10 minutes, an hour, a day, a week, or even a month. Nonetheless, when put into a larger time-frame a criminal event is quite profitably viewed as the realization of a point process distributed over time and space, and we do so in Section 5. What complicates the modelling of a large number of crimes is the interpenetrating social networks linking offenders and victims, both within a single incident and across several incidents, and giving rise to multiple offending and multiple victimization. Reiss (1977a) describes some of the impact of such networks and associated group structures on crime rates with special attention to the implications for measuring the effects of deterrence and incapacitation. The stochastic structure of criminal social networks and the resulting lack of independence of criminal incidents

also has potentially important implications for both the design and analysis of victimization surveys. It is for this reason that we discuss some first steps in the stochastic modelling of victimizations for individuals over time in Section 5.

How one records crime is a function of one's perspective. A single criminal incident or social encounter can involve one or more offenders, one or more victims or possibly no victims at all, and multiple violations of the law leading to multiple indictments of a single offender or several offenders who have participated in the event. There may even be mutual offending and victimization, e.g. in cases of assault. Thus a particular configuration of crimes aggregated over a given time period may well look dramatically different when viewed from the perspective of offense rates as opposed to victimization rates, and neither set of rates is likely to reveal the true nature of the criminal events that have taken place.

A single hypothetical example can illustrate the complexity associated with criminal incidents and the manner in which they are recorded. A young couple living in the household of the woman's parents in Stamford, Connecticut go to New York City on December 31 to celebrate New Year's Eve. They park their car in a lot on the east side of Manhattan and have a leisurely dinner at a nearby restaurant. After dinner when they return to their car, they are accosted by five young males just outside the parking

lot and are taken into an adjacent alleyway, at approximately 11:00 P.M. One of the youths threatens the couple with a revolver, and the other four take turns raping the woman. When the woman resists, one of the youths assaults her with a knife, and then he also assaults the man. Following the acts of rape the youths take the woman's purse and the man's wallet, and they appear to flee. It is now about 1:00 A.M., January 1. The couple have to travel several blocks to report the incident to the police. When they finally return to the parking lot with a police officer at 3:00 A.M., they discover that their automobile is missing. A week later three young males are stopped by the police in Newark, New Jersey driving the couple's car through a red stoplight and they are arrested.

The incident just described involved five offenders, two victims, three arrests and numerous offenses including forcible rape, robbery, aggravated assault, and motor vehicle theft. It spanned several hours (and two calendar years!) and took place in at least two locations. How would it be classified by various recording systems?

Let us begin with the police record of the event as it is transmitted to the FBI for use in its Uniform Crime Reports (UCR). In a multiple offense situation, the police classify each offense, and then locate the offense that is highest on the list of what is known as Part I Offenses (the ranking is criminal homicide, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft). The highest offense is entered

and the others are ignored. Multiple offenses need to be separated in time and place to lead to multiple entries in the UCR. The exception to this rule involves crimes against the person (criminal homicide, forcible rape, and aggravated assault) where one offense is entered for each victim. Thus the UCR record will contain one offense of forcible rape (against the woman) and one offense of aggravated assault (against the man). Had the youths only robbed but not assaulted the man, there would only be one offense entered. These offenses would be recorded by the New York City police, and I am unclear as to which day (and thus which year) they will be attributed to. The UCR record will also show that the offense(s) have been cleared (i.e. "resolved") by the arrest of the three youths in New Jersey. Although this event led to one or two UCR offenses, it might well lead to the prosecution of the five youths on up to a total of five counts of rape, 10 counts of aggravated assault and of robbery, and 5 counts of motor vehicle theft.

Suppose now that the couple's household is chosen as part of the NCS so that the event will also be recorded from the victims' perspective. Both the man and woman would be interviewed separately and the NCS would record two victimizations in December: one for the woman "assaultive violence with theft - rape", one for the man "assaultive violence with theft - serious assault with weapon." Even if the man had only been robbed but not assaulted there would still be two victimizations recorded (as compared with a single

offense). Moreover, because of the separation of household victimizations from individual victimizations, when the woman's father reports the household victimizations, he may well report the theft of the car separately, and the month of victimization may be given as January, and thus it could go into a separate calendar year.

In summary, our single criminal incident involving 5 offenders and 2 victims, leads to 1 or 2 offenses recorded in New York and 2 or 3 victimizations recorded in Connecticut. The perspectives are clearly different, and so too are the records of the event.

Because a large proportion of criminal incidents are never reported to the police, the discrepancy between all criminal offenses and those reported to the police has been described by Biderman and Reiss (1967) as the "dark figure" of crime, and one of the original purposes of victimization surveys was "to bring more of the dark figure to statistical light." Biderman and Reiss go on to note:

> In exploring the dark figure of crime, the primary question is not how much of it becomes revealed but rather what will be the selective properties of any particular innovation for its illumination. As in many other problems of scientific observation, the use of approaches and apparatuses with different properties of error has been a means of approaching truer approximations of phenomena that are difficult to measure.
>
> Any set of crime statistics, including those of the survey, involves some evaluative, institutional processing of people's reports. Concepts, definitions, quantitative models, and theories must be adjusted to the fact that the data are not some objectively observable universe of "criminal acts," but rather those events defined, captured, and processed as such by some institutional mechanism.

Much controversy has centered on the comparability of police statistics on offense rates and NCS survey statistics on victimization rates (e.g. see Biderman 1967, Biderman and Reiss 1967, Penick and Owens 1976, p. 152-4, and U.S. Department of Justice 1967b), but the utility (or lack thereof) of the NCS data for such comparisons should not obscure the richness of information about victimization available in the NCS. It is for this reason that the NCS data must be collected and organized in a manner that will make it amenable to standard forms of statistical analysis. Otherwise the rich veins of information, on such topics as high-risk segments of the population and multiple victimization, or the way that deviance is perceived and dealt with in various social contexts, may never be mined.

3. DESIGN OF THE NCS

3.1 Sample Design

The NCS is a sample survey of households and their occupants, and as such it closely resembles the Current Population Survey (CPS), which is also conducted by the Bureau of the Census, in almost all aspects. In fact, descriptions of the designation of housing units for the CPS (e.g. see Thompson and Shapiro 1973) are almost identical to those for the NCS (e.g. see U.S. Department of Justice 1976a, 1976b), the major exceptions being the sample sizes, the interview schedules, and the panel and rotation group structures.

The structure of the NCS is that of a stratified multistage cluster sample. The first stage consists of dividing the U.S. into approximately 2000 primary sampling units (PSU's) comprising counties or groups of contiguous counties. The PSU's are then separated into 376 strata and one PSU is selected from each stratum with probability proportional to population size. Within each PSU so selected, a systematically chosen group of enumeration districts is selected, and then clusters of approximately four housing units each are chosen within each enumeration district. For 1973, this process led to the designation of about 80,000 housing units, and interviews were obtained from occupants of about 65,000. Most of the remaining designated housing units were vacant or otherwise ineligible for inclusion in the NCS.

The basic sample is divided in 6 subsamples or rotation groups of a little over 10,000 households each. (Actually there are 7 subsamples, but the data for the newest one are not incorporated into the reported rates. Rather these data are used for bounding purposes, as described in Section 3.3.) The occupants 12 years of age or older are interviewed at six-month intervals for a total of three years. Every 6 months a new rotation group enters the sample and the "oldest" existing rotation group from the previous sample is dropped. Each rotation group is divided into 6 panels, with one panel being interviewed in each month of the 6-month period.

For estimating various rates a series of weights and adjustment procedures are applied to the raw data. The weighting procedures are standard practice for surveys of this sort and are basically designed to adjust for the differential probabilities of including various household locations in the survey, and to reduce bias and variance of sample estimators. The final adjustment involves the use of ratio estimation so that the distribution of individuals (or households) in the sample is in accord with independent estimates of the current population in each of 72 age-sex-race categories.

By reporting only adjusted rates, for both the NCS and the CPS, Census has removed from public scrutiny many of the actual defects of the sample design when it is actually implemented. Since all aggregate counts have essentially the same totals for various categories we can never tell when a given sample is badly off the mark, and in what directions.

Although the NCS is basically a sample of household locations, at the same time it yields both a sample of households or families and a sample of individuals. Household locations are of little substantive interest in the study of victimization. While the NCS allows for the study of differential rates of victimization by type of household location (e.g. house, apartment, rooming house, mobile home), not one of the 100 tables in the LEAA report for 1973 (U.S. Department of Justice, 1976b)

deals with such information. The primary reason that the NCS is a sample of locations rather than households or individuals appears to be because Census has available a detailed frame only for locations.

The NCS primarily measures victimization while the CPS primarily measures employment and unemployment. Since both unemployment and victimization are relatively rare phenomena, a naive person might suggest a sample design that has proved successful for measuring unemployment should, with only minor modifications, do a good job of measuring victimization. Such a suggestion is naive because, among other things, it ignores the considerable knowledge we have available regarding crime and its physical as well as socio-economic characteristics. In central cities, crime rates vary dramatically from block to block, and a limited amount of field work might lead to cluster boundaries that differ dramatically from those that would seem appropriate for unemployment. It may well be that the NCS sampling plan is most sensible given budgetary constraints, but an exploration of alternatives and variants to the current plan should probably be included in Census' research, development, and evaluation program.

3.2 Questionnaire Design

The questionnaire administered every six months at each household location consists of two parts; (a) a basic screen, and (b) crime incident reports. The basic screen includes household

location information, household or family information, the personal characteristics of all of the individuals in the household (who may change from interview to interview), plus household or individual screen questions on crime. The report of the Panel for the Evaluation of Crime Surveys (Penick and Owens 1976) gives a detailed critique of the basic screen, and we refer the interested reader to their discussion. For each crime incident detected by the screen, a crime incident report containing answers to almost 100 questions is completed.

The questionnaire distinguishes between individual identifiable incidents, and series of at least three similar incidents which the respondent is unable to separate in time and place of occurrence. For individual victimizations, the questionnaire records the month in which the crime took place, but for series victimizations the respondent only needs to indicate the quarter(s) in which the incidents took place (i.e. spring, summer, fall, winter), the number of incidents (3-4, 5-10, 11+, or don't know), and the details for the most recent event in the series. We discuss the distinction between single and series victimization in more detail in Section 4, where we note how Census treats series victimizations and why we believe series victimizations should be the topic of extensive analytical investigation. What is unclear to us from published documents and various unpublished memoranda is the extent to which series victimization is a true phenomenon or an artificial construct resulting from the NCS questionnaire design.

Not only does the NCS questionnaire solicit information on the details of an incident, the offender, and any resulting physical injury and how it was treated, but it also inquires whether the incident was reported to the police and if not, why not.

3.3 Reference Period and Bounding

One of the most crucial problems in the design of a victimization survey is eliciting accurate information on the time of occurrence of criminal incidents. The problem has at least two components:

(a) Recall Decay. The longer the time lapse between a criminal incident and the date of interview, the greater the probability that the event will not be reported to the interviewer.

(b) Telescoping. Events occurring in one time period can be reported as occurring in a different one. The displacement of telescoped events can be forward or backward in time.

It is especially difficult to model recall decay and telescoping, since much evidence seems to point to differential rates of decay and telescoping for different types of crimes, and for different types of respondents. Moreover, there can be no check on a crime that has never been reported, either to the police or the NCS. Thus the only way to get a handle on these two phenomena is via a sample of crimes reported to the police and the subsequent inclusion of victims of these reported crimes in a victim survey. Such "reverse record checks" were part of the pretests

of the NCS survey instrument (see U.S. Department of Justice 1972, 1974). The problem with drawing inferences from reverse record checks is that they are aimed at data which are missing from the victimization survey, but which are not missing at random (see Rubin 1976 for a discussion of the importance of the missing at random assumption).

A consideration of both recall decay and telescoping is necessary for the determination of the optimal reference period for a victim survey. The NCS reference period is 6 months, and Census uses the first interview and 6-month period of a household location for bounding, i.e. establishing a time frame to avoid duplication of incidents in subsequent interviews. For a detailed study of the effects of bounding on telescoping see Murphy and Cowan (1976). A major problem in the design of the NCS arises because the bounding procedures bound household locations, not households nor individuals. If one household replaces another during the course of the 3-year period during which a location is included in the NCS sample, there is no bounding for the new household or of its members as individuals. Murphy and Cowan (1976) report that unbounded households in returning rotations groups comprise (for 1974-75) 13.3% of the interviewed sample. In addition only about 95% of the interviews in the bounded households are themselves bounded due to considerable transience for households in heavily urban areas. As a result, as few as 20% of the individuals over a 3-year period in a given set of household

locations may produce complete victimization records for the period. These design characteristics drastically impair the utility of the NCS data for longitudinal analysis of individual victimization profiles.

Considerable methodological interest is centered on the differences in victimization experience for migrants and non-migrants. In addition to follow-up studies of out-migrants (which are quite costly), it seems reasonable to do special analyses of the in-migrants to the sample locations since their data is already in the NCS (see Penick and Owens 1976 and Reiss 1977b). For every out-migrant household there is an in-migrant one. Of course the current lack of bounding for in-migrants would complicate such analyses, but it should be feasible to do a special study of in-migrants where a bounding period would be included along with additional interviews beyond the standard 3-year period for the household location.

4. PUBLISHED ANALYSES OF THE NCS DATA

Not only does the formal responsibility for the design and execution of the NCS lie with the U.S. Bureau of the Census, but the analysis of the collected data is also the responsibility of a small staff of Census employees. This analysis by LEAA and Census involves the periodic preparation of two and three-dimensional cross-tabulations of estimated victimization rates and estimates of their standard errors. The cross-tabulations produced are basically those requested in advance by professional staff at LEAA,

and not as a result of a more detailed and complex statistical analysis.

Suppose for simplicity that NCS employed a simple random sample and that the data (which are primarily categorical in nature) for any year were analyzed using some variant of loglinear model analysis for a k-dimensional cross-classification (e.g. see Bishop, Fienberg, and Holland 1975). Then one of the implications of finding a model that gives a good fit to the data would be that the k-dimensional table may be succinctly summarized by a series of tables of smaller dimension, from which the original table can be reconstructed with essentially zero information loss. Such analyses can thus provide a rationale for reporting certain cross-tabulations and not others. This point is described in more detail in Fienberg (1975). Even though the NCS does not employ simple random sampling, the idea of careful statistical analyses leading to the choice of cross-tabulations to be published is one which should be considered more seriously by LEAA and Census.

How many reports has LEAA published on the results of the NCS national household sample? As of December 1976, several preliminary but only two final reports had been released: a 162-page report on the 1973 survey (U.S. Department of Justice 1976b), and a much briefer 73-page report comparing findings for 1973 and 1974 (U.S. Department of Justice 1976a). Since both final reports also contain data on separate commercial surveys, the interested reader

is left with very slim pickings from what appeared to be a sumptuous meal. Moreover, these two reports contain only weighted data or proportions. No raw counts are available. Thus it is almost impossible for the skilled statistician to do extensive secondary analysis of the published data.

When preliminary versions of the 1973 report were distributed by LEAA, several investigators noted that series victimizations were not included in the computation of any published rates or calculations. Thus all reported numbers and rates of victimization may be severe underestimates. For example, LEAA estimated for 1973 (U.S. Department of Justice 1976b) that there were approximately one million series victimizations in the personal sector and just over 20 million victimizations not in series. A series consists of three or more victimizations, and an average of 5 victimizations per series is likely an underestimate for the NCS data. (Some calculations based on an unpublished tabulation suggest that the average is in excess of 6 victimizations per series.) This then means that at least 20% of all victimizations in the personal sector have been excluded from the reported calculations. This matter becomes even more serious when we note that in 1973 46.3% of all personal series victimizations involved crimes of violence while only 26.6% of all victimizations not in series, and that series victimizations may have accounted for over one-third of all crimes of violence.

We note that despite the panel structure of the survey, LEAA has yet to make use of the full longitudinal structure of the data base. The construction of a panel tape tracking individuals and households over time was not deemed to be a central goal of the NCS, and the preparation of such a tape was only belatedly arranged through a contract with a group at a private university. It might be argued that the panel structure of the NCS sample is intended to handle certain technical problems and to give more accurate year to year comparisons, and not for longitudinal analysis of individual files. This can be true in only this narrowest of senses because without a detailed longitudinal analysis we can never know whether the aggregate annual reported victimization rates are at all accurate. For example, Reiss (1977b), reporting on some preliminary longitudinal analyses, notes that highly victimized individuals are much more likely to be out-migrants than those with low victimization rates, and series victims are more likely to move than non-series victims. Moreover, a high percentage of individuals reporting series victimizations in a given 6-month period report no victimizations in the subsequent 6-month period. These observations call into question the accuracy of the published victimization rates.

5. MODELING VICTIMIZATION

To understand reported annual victimization rates and the implications of changes in them from one year to the next, we need a detailed understanding of how victimization varies among individuals and subgroups within the population. This detailed understanding will necessarily have to come from the analysis of disaggregated data, and of individual victimization records over time. Such analyses will be complicated by the complex structure of the NCS sample design, but the effects of stratification and clustering on analyses will vary greatly from problem to problem. For many problems the use of unweighted data may well simplify the modeling process. This is clearly the case if we are interested in the structure of individual reported victimization patterns over time.

The Panel for the Evaluation of Crime Surveys gives several suggestions for analytic research on the existing NCS data. One of these suggestions deals with the relationship between series victimization and multiple victimization, a topic we discussed in Section 4. To investigate this relationship, however, we need models for the occurrence of victimizations over time, and we propose one such model here in Section 5.2. A second suggestion deals with analyses to investigate under- and overreporting of incidents as they relate to the month of incident and the month of interview. We take up some aspects that need to be considered in such analyses next in Section 5.1.

5.1 Reporting Biases and Time-in-panel

For several characteristics on which data are collected in the Current Population Survey, Bailar (1975) notes that there is a higher level for the first interview than for succeeding ones, and so on. The effect of such variation is usually referred to as "rotation group bias", and there is reason to expect such biases in the NCS data as well. In the NCS the rotation group bias problem is compounded by several factors including the elapsed time between the incident and the interview (recall that interviews provide data for the preceding 6-month period).

What we would like to do is develop a model which compares the victimization rates for specific crimes for a series of reference months as a function of the number of interviews, the time-lag from incident to interview, and other possibly relevant temporal variables. We build up to this in stages.

In Table 3-1 we show the list of panels being interviewed by month of collection for a full 3-year collection cycle, where the months have been labelled from 31 to 66. Panels 1-6 form a subsample that was first interviewed in months 1-6 (we ignore the initial interview for bounding purposes here) and leaves the sample after the interviews in months 31-36. Note that the difference between the month of collection and the number of a panel being interviewed equals the number of months the panel has been in the sample (time-in-panel). All three variables bear examination in terms of their effect on reported rates. The time-in-panel variable

yields the rotation group bias information, while month of collection measures seasonality and other unique temporal effects, and panel number represents temporal characteristics and effects unique to those that entered the sample at the same time. The formal identity linking these three variables is the same as that linking age, period, and cohort as described in Fienberg and Mason (1977), and any model using all three as independent variables needs to take into account the identification problem associated with the linear components of the effects.

Table 3-1 goes about here

Since each interview collects data for the preceding 6-month period, for each reference month there are a total of 36 distinct panels which provide data. For example, panels 1, 7, 13, 19, 25, and 31 provide data with a one-month lag for month 30 during collection month 31; panels 2, 8, 14, 20, 26, and 32 provide data with a two-month lag during collection month 32; and so on. Thus the ensemble of 36 victimization rates for a given reference month can be modeled as a function of month of collection, time lag to reference month, panel number, and time in panel (as well as various additional independent variables such as education and race if we wish to compare subgroups of the sample).

Of course we need to model several reference months simultaneously if we are to use all of the independent variables at once. If we in addition use reference month as an independent variable

then we have an additional identification problem related to the identity involving reference month, collection month, and time-lag until interview.

To analyze and model data using the variables just described we need to know whether we can treat the data for different reference months from the same panel as being independent. Moreover, it is unclear whether we should use rate as the response variate or counts of victimizations (e.g. the number of respondents with 0, 1, 2, . . . victimizations), and whether we should use weighted or unweighted data.

Models of the sort we have just described need to be explored carefully if we are to get a proper handle on such problems as rotation group bias, memory decay associated with recall, etc. Modeling these phenomena separately (as in Bailar 1975, and Finkner and Nisselson, 1977) when they in fact occur simultaneously should only be the first step in an analysis, since it may lead to improper inferences unless there are order-of-magnitude differences in the sizes of their effects. What is especially troublesome with any attempt to model these phenomena is that we can only deal with individual victimizations, and not series even though the latter may make up a sizeable proportion of the total reported victimizations in a given period.

5.2 A Model for Multiple Victimizations Over Time

Most of the models that have been proposed for victimization

assume that each individual has an "annual" victimization rate λ_i for crime type i, and that the expected number of victimizations the individual will experience for crime type i in a fixed period of time, T, is simply $\lambda_i T$. This is, of course, the expected number if we assume that victimizations follow a Poisson process. Since victimization is a rare event, in order to test the Poisson model we need to pool individuals into groups expected to have similar values of λ_i. Those victimization studies that have looked at victimization distributions for fixed periods of time and for subgroups of the population typically find that the Poisson model gives a poor fit. This may be an artifact of the data collection procedure, it may be a result of not using a fine enough disaggregation, or it may in fact be the result of the inappropriateness of the Poisson process.

One more general structure for modeling victimization as a point process is the semi-Markov process, which includes the Poisson process as a special case. In this structure we view victimization as a point process $\{Y(t), t > 0\}$ where $Y(t) = j$ if the individual were last a victim of crime type j. If the process is semi-Markov (see for example, Çinlar 1975), then it has transition probabilities

$$p_{ij}(t) = \Pr \{Y(t) = j \mid Y(0) = i\} , \qquad (5.1)$$

where i and j run over the possible types of crimes, say $1 \leq i, j \leq r$. These transition probabilities can be expressed directly in terms of two sets of quantities:

(a) a matrix of one-step transition probabilities governing a discrete-time Markov chain, $\underset{\sim}{M} = \{m_{ij}\}$, which represent an individual's "victimization propensities" given his current victimization state,

(b) a family of waiting time distributions, $\mathcal{F} = \{F_1(t), F_2(t), \ldots, F_r(t)\}$, characterizing the inter-victimization intervals and depending on the last type of victimization.

The transition probabilities are the unique solution of the system of equations

$$p_{ij}(t) = \delta_{ij}[1 - F_i(t)] + \sum_{k=1}^{r} \int_0^t f_i(s) m_{ik} p_{kj}(t-s)ds \qquad (5.2)$$

where $i,j = 1, 2, \ldots, r$,

$$\delta_{ij} = \begin{cases} 1 & \text{if } i = j \\ 0 & \text{if } i \neq j \end{cases} \qquad (5.3)$$

and $f_i(t)$ is the probability density corresponding to the distribution function $F_i(t)$. When the distributions $F_i(t)$ are exponential the process reduces to a time-homogeneous Markov one, and when, in addition, the probabilities $\{m_{ij}\}$ are independent we get a set of Poisson processes.

In order to use this general semi-Markov model for the NCS data we need to see how the one-month-at-a-time data collection framework of the NCS can be embedded in the structure of the continuous time model. This problem resembles one explored by Singer and Spilerman (1974, 1976a, 1976b), who have used the semi-Markov process model of equations (5.1) and (5.2) for investigating occupational mobility. In their work they have placed special emphasis

on the embeddability of fragmentary multi-wave panel data into a class of continuous time Markov models, and the identification problem within that class of models.

The use of this class of models in the context of the NCS is complicated by the fact that as few as 20% of all individuals have full three year records. Moreover, it is unclear whether we need to take into account the complexities of the sample design when we try to model the victimization histories of individuals with common socio-demographic and geographic characteristics. A final complication in the NCS data is the existence of series victimizations, which illustrate a strong propensity for rapid and repeated victimization of a specific type. Analyses based on underlying continuous time models certainly should include both series and separate individual victimizations.

6. DISCUSSION

The two models described in the preceding section have not been exploring with the NCS data, even in a preliminary form. They do, however, illustrate the problems involved in the analysis of data from the NCS when the purpose of the analysis is to provide estimates of aggregate victimization rates. While some have argued that modeling of this sort is unrelated to the primary objectives of the NCS, we disagree. First, we believe that an understanding of the basic structure of the panel data produced by the NCS is crucial to a proper evaluation of aggregate victimization rates.

Second, the detailed stochastic modeling of individual records is required to directly meet one of the NCS objectives described in the Introduction of this paper: to identify high-risk subgroups and to estimate the rate of multiple victimization. Third, a reading of various documents about the NCS makes clear that it is in fact a multi-purpose survey, and substantive issues and concerns need to be properly articulated so that the NCS design may be appropriately modified.

Because the NCS is similar in sample design to many other large-scale social surveys such as the CPS, the National Housing Survey, and the National Assessment of Educational Progress, it shares with these other surveys various methodological problems associated with data analysis and inference. For example, the weighting procedures used to get aggregate victimization rates and estimates of standard errors, are not necessarily appropriate for other analytical purposes. To solve these problems, statisticians must develop variants of various multivariate techniques appropriate for the analysis of data from complex surveys. At the same time they must work toward the development of survey designs that are especially amenable to classes of analytical purposes, or at least to specific forms of analysis.

Our evaluation of the NCS is well summarized by the following excerpt from the Report of the Panel for the Evaluation of Crime Surveys (Penick and Owens 1976, p. 152):

The panel has found much to commend, and much to criticize, in the design and execution of the NCS to date. We have argued that a very great amount of methodological and developmental research must be done, and many changes in existing procedures must be made, if certain of the specific initial objectives of the surveys are to be accomplished. The panel also maintains, however, that those objectives themselves need further scrutiny and that a subtle but fundamental change in the official concept of victimization surveying is necessary if the potential value of this relatively new research method is to be fully realized.

Table 3-1: An Illustration of the NCS Panel Rotation Structure

Collection Month	Panels Being Interviewed										
31	1	7	13	19	25	31					
32	2	8	14	20	26	32					
33	3	9	15	21	27	33					
34	4	10	16	22	28	34					
35	5	11	17	23	29	35					
36	6	12	18	24	30	36					
37		7	13	19	25	31	37				
38		8	14	20	26	32	38				
39		9	15	21	27	33	39				
40		10	16	22	28	34	40				
41		11	17	23	29	35	41				
42		12	18	24	30	36	42				
43			13	19	25	31	37	43			
44			14	20	26	32	38	44			
45			15	21	27	33	39	45			
46			16	22	28	34	40	46			
47			17	23	29	35	41	47			
48			18	24	30	36	42	48			
49				19	25	31	37	43	49		
50				20	26	32	38	44	50		
51				21	27	33	39	45	51		
52				22	28	34	40	46	52		
53				23	29	35	41	47	53		
54				24	30	36	42	48	54		
55					25	31	37	43	49	55	
56					26	32	38	44	50	56	
57					27	33	39	45	51	57	
58					28	34	40	46	52	58	
59					29	35	41	47	53	59	
60					30	36	42	48	54	60	
61						31	37	43	49	55	61
62						32	38	44	50	56	62
63						33	39	45	51	57	63
64						34	40	46	52	58	64
65						35	41	47	53	59	65
66						36	42	48	54	60	66

REFERENCES

Bailar, Barbara A. (1975), "The effects of rotation group bias on estimates from panel surveys," Journal of the American Statistical Association, 70, 23-30.

Biderman, Albert D. (1967), "Surveys of Population Samples for Estimating Crime Incidence," The Annals of the American Academy of Political and Social Science, 374, 16-33.

———, and Reiss, Albert J. Jr. (1967), "On Exploring the 'Dark Figure' of Crime," The Annals of the American Academy of Political and Social Science, 374, 1-15.

Bishop, Yvonne M.M., Fienberg, Stephen E., and Holland, Paul W. (1975), Discrete Multivariate Analysis: Theory and Practice, Cambridge: MIT Press.

Çinlar, Erhan (1975), Introduction to Stochastic Processes, Englewood Cliffs, New Jersey: Prentice-Hall.

Fienberg, Stephen E. (1975), "Perspective Canada as a Social Report," Social Indicators Research, 2, 153-174.

———, and Mason, William (1977), "Identification and estimation of age-period-cohort models in the analysis of discrete archival data," University of Minnesota Technical Report No. 286.

Finkner, A.L. and Nisselson, Harold (1977), "Some statistical problems associated with continuing cross-sectional surveys," this volume.

Hood, Roger and Sparks, Richard (1970), Key Issues in Criminology, New York: McGraw-Hill.

Murphy, Linda R. and Cowan, Charles C. (1976), "Effects of bounding on telescoping in the National Crime Survey," American Statistical Association Proceedings, Social Statistics Section, Part II, 633-638.

Penick, Bettye K. and Owens, Maurice E.B. (eds.) (1976), Surveying Crime, Panel for the Evaluation of Crime Surveys, Washington, D.C.: National Academy of Sciences.

Reiss, Albert J. (1977a), "Understanding changes in crime rates," unpublished manuscript.

——— (1977b), Personal Communication.

Rubin, Donald B. (1976), "Inference and Missing Data," Biometrika, 63, 581-592.

Singer, B. and Spilerman, S. (1974), "Social Mobility Models for Heterogeneous Populations." In Herbert Costner (ed.), Sociological Methodology 1973-74, San Francisco: Jossey-Bass, pp. 356-401.

——— (1976a),"The Representation of Social Processes by Markov Models," American Journal of Sociology, 82, 1-54.

——— (1976b), "Some Methodological Issues in the Analysis of Longitudinal Surveys," Annals of Economic and Social Measurement, 5, 447-474.

Thompson, Marvin M. and Shapiro, Gary (1973), "The Current Population Survey: An Overview," Annals of Economic and Social Measurement, 2, 105-129.

U.S. Department of Justice, Law Enforcement Assistance Administration (1972), San Jose Methods Test of Known Crime Victims, Statistics Technical Report No. 1, Washington, D.C.: U.S. Government Printing Office.

——— (1974), Crimes and Victims: A Report on the Dayton-San Jose Pilot Survey of Victimization, Washington, D.C.: U.S. Government Printing Office.

——— (1976a), Criminal Victimization in the United States, A Comparison of 1973 and 1974 Findings, No. SD NCP-N-3, Washington, D.C.: U.S. Government Printing Office.

——— (1976b), Criminal Victimization in the United States 1973, No. SD NCP-N-4, Washington, D.C.: U.S. Government Printing Office.